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# IS THERE SCIENTIFIC BASIS FOR FLYWAY MANAGEMENT?

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In attempting to discuss the scientific basis of waterfowl management by flyways, I have written this paper as a review of the biological principles we are now using or hope to use. I would particularly like to appraise the practical difficulties that we face in placing waterfowl harvests on a scientific basis.

Here at the beginning, I would like to restrict my use of the word *science* to accumulated and accepted knowledge which has been systematized so as to permit the discovery of general truths. An immature science often involves the slow accumulation of facts which will ultimately be fitted into a mosaic permitting one to perceive the operation of general laws. In a mature science, these laws are sufficiently perceived so as to permit one to make predictions about the future. Waterfowl biology has, for instance, attained its greatest degree of maturity in its grasp of the migratory behavior of ducks and geese, in its awareness of many of the migration routes that are used, and in its understanding of the species composition of birds on the breeding and wintering grounds and at migratory concentration points. It is scarcely necessary to point out that this body of knowledge formed an important scientific foundation for the initial acquisition of our great national wildlife refuges. It is used by each state in the selection of dates for the annual hunting season. It governs the distribution of law-enforcement officers. In short, it permits planning for the future.

Parallel to this science is an *art* which consists of a collection of skills acquired by experience and observation. The art of waterfowl management extends to the management of man, as when we continue to plug loopholes in our laws so as to simplify our control of poaching. It extends to the management of habitat, as when we choose a quiet night in which to burn *Phragmites*, or elect to gamble by plowing on a given day, or knock down a field of corn in order to hold some newly arrived geese.

A primary aim of modern game management today lies in its hope of changing as many of its techniques as possible from an art to a science of game management (McCabe 1954). A few years ago, Clarence Cottam (1949) and Harlow B. Mills (1951) each reviewed the factual basis of waterfowl management and emphasized the extremely wide variety of information that still needs to be discovered before the management of these populations can be said to have attained the level of a science. Are we practicing the art of waterfowl management today? Or are we well on the road to making it into a science? Are there practical reasons why some aspects of waterfowl management will always remain an art to which science can make only a minor contribution? These are the questions I would like to face in this paper.

## HABITAT MANAGEMENT

Management of waterfowl habitat today is partly an art and partly a science. The scientific basis is clearest in the dependence on hydrological principles for the availability of water and on the engineering sciences for the permanency of dikes, spillways and the like. Where habitat manipulation has had to face biological problems, waterfowl managers have borrowed from agriculture, range management, plant and animal ecology, and animal psychology when they could. In many cases, however, there are no ready-made formulas, and our whole habitat-management program has, in a sense, been a continuous research project in which practical operations have constantly served as experiments. One firm foundation for this program was the early and long-continued work of the Bureau of Biological Survey in ascertaining the food habits of waterfowl and in exploring methods of propagating aquatic plants (McAtee 1911 *et seq.;* Cottam 1939; Martin and Uhler 1939).

The complexity of aquatic-habitat management is well illustrated in the varied uses of drawdowns and grazing by waterfowl managers. A drawdown may destroy food plants on a TVA lake (Steenis 1950b), or serve as the initial step in the creation of a dense stand of valuable smartweed in Michigan (Pirnie 1935:213), or make beds of submerged aquatics available to puddle ducks in the South (Griffith 1948). Summer grazing by livestock may be successful in readying a Wisconsin field for autumnal geese (Hughlett, personal communication); its elimination on a unit of short-grass country may increase the number of duck nests as much as 30 per cent (Griffith 1948). Used judiciously, grazing may create edge effects that will be helpful as brood cover; used carelessly, it can completely eliminate softstem bulrush and reduce millet and smartweed by 80 per cent (*ibid.*). There are important ecological principles which govern such changes in the production or availability of food; but when much of the operation depends for its success upon the experience of the manager and his intimate familiarity with his marsh, the management process must at least be considered as much of an art as it is a science.

The use of upland farming to supplement natural supplies of waterfowl food has become an increasingly important principle in our adding to the carrying capacity of many waterfowl refuges (Griffith 1948, Horn 1949, Steenis 1950b, Atkeson and Givens 1952). I have heard agriculture characterized as half art and half science. While this division undoubtedly varies with the type of farming, it clearly implies that the relatively recent practice of waterfowl-habitat management will require a lot more research before it stands upon its feet as a series of scientific procedures.

At the present time, research is serving to broaden our management of waterfowl habitat by fundamental studies of the ecology of the aquatic plants we require (Chamberlain 1948, Crail 1951, Cottam and Bourne 1952) and of the ability of these plants to carry given numbers of ducks or geese (Lynch, O'Neil and Lay 1947; Griffith 1948; Horn 1949).

It is likewise succeeding in giving us new tools, as in the use of herbicides to control aquatic weeds (Eicher 1947, Gerking 1948, Steenis 1950a). It is evaluating older tools, like burning, for their effects on waterfowl foods (Singleton 1951). And it is testing new techniques, like the transplantation of ducks and geese to new or vacated breeding grounds (Johnson 1947, McCabe 1947, Bellrose 1953a), the artificial creation of small-water areas (Bue, Blankenship and Marshall 1952, Brumsted and Hewitt 1952), extensive systems of nesting boxes (Frank 1948, McLaughlin and Grice 1952, Bellrose 1953a), and the use of dynamite in the creation of edge (Mendall 1949).

Although this sums up to an increasingly impressive record of technological achievement, it is true, as Griffith (1948) says, that "there is no hard and fast rule that can be recommended towards the treatment of a particular tract of wildlife habitat to increase its productivity." In effect, the art of marsh management will always be wedded to the science of marsh ecology. Certainly, one's net impression of aquatic-habitat manipulation today is that it is proceeding on a rapidly expanding scientific basis. A major problem today lies not in how aquatic habitats shall be managed, but where and with what dollars. It is in this administrative strategy that flyway management can make an important contribution.

### *CONTROL OF NATURAL MORTALITY*

Techniques of reducing nonhunting mortality in waterfowl are evolving more slowly. Just to put the picture at least into a partial focus, I would estimate that less than one million waterfowl simply vanish each year from this continent.

Where natural mortality has been locally concentrated, research workers have a chance to meet the problem head on. Mortality from a botulism outbreak can now be reduced by about 95 per cent with techniques such as the herding of birds, removal of the dead, and changing water levels (Rosen and Bischoff 1953). The success of these practices has a firm scientific basis contributed by many investigators and stands as a proud achievement. Aside from nest predation and the occasional outbreak of lead-poisoning, fowl cholera, or algal poisoning, the evidence for most nonhunting mortality in waterfowl disappears so rapidly in the field as to make the progress of research tenuous and extremely difficult to plan.

The report of Ducks Unlimited (Canada) that over 5,000,000 crows and blackbirds have been destroyed since 1937 is an interesting example of an attempt to reduce mortality at the nest stage. This program has its basis anchored in research that Kalmbach (1937) carefully labelled as "preliminary studies." Between the original finding and the present control program, two steps are missing. One of these lies in an evaluation of the net effect of the predation in reducing the total number of young raised; the second lies in controlled experiments that test the actual effect of a crow- and blackbird-control program upon the production of waterfowl. I am here neither disparaging nor advocating crow control. I am observing that the destruction of so many birds does not rest upon a sound published scientific basis.

Other techniques to reduce nesting losses include the creation of islands for geese (Johnson 1947), elimination of grazing by livestock (Griffith 1948), control of water-level when this is possible (Mendall 1949), the artificial creation of flood-proof nesting platforms for geese (Yocum 1952), and development of a predator-proof nesting box for wood ducks (*Aix sponsa*) (Bellrose 1953a). While these have a somewhat local application, the wood duck box also has state-wide possibilities in creating niches for this species in stands of immature timber, as McLaughlin and Grice (1952) have shown. The new box deserves a wide-scale tryout in the Atlantic and Mississippi Flyways, with boxes concentrated on watersheds which can be checked for brood production, and comparisons made with other unmanaged watersheds left as controls.

Control of natural mortality is characterized by the regional importance of the factors to be faced: pollution in the industrial East, raccoons (*Procyon lotor*) in the Mississippi Valley, crows (*Corvus *brachyrhynchos*) on the prairies, and botulism in the Far West. There is a clear need to integrate our management of the wood ducks-raccoon problem in the Atlantic and Mississippi Flyways, and to develop a sound approach to the crow in the Central Flyway. The coordination of ideas and techniques on botulism I take as an accepted fact. Progress in the control of natural mortality will undoubtedly be accelerated by flyway-management plans. The progress will be slow, but it will, I think, be concrete.

Two other phases of waterfowl management involve control of the harvest taken by hunters and control of depredations. The latter is a relatively new and increasingly important problem on which Horn (1949) has reported progress on the wintering grounds of California and on which Hochbaum, Dillon and Howard (1954) have reported some progress on the breeding grounds in Manitoba. Because the harvest is of paramount interest to so many hunters, and because its biological basis is so little understood, I would like to devote the rest of my paper to a discussion of this subject.

### WHAT BIOLOGICAL PRINCIPLES UNDERLIE WATERFOWL HARVESTS?

In attempting to answer this question, I am deliberately omitting any mention of harvests which are largely determined by economic factors such as real or potential crop depredations by waterfowl on their breeding grounds or their wintering grounds. This phenomenon is regional in character, and the harvest is, of course, designed to prevent the birds from attaining a given population level. The more normal waterfowl regulations are aimed in principle not at merely providing interest on a capital investment but at yielding the maximum interest without damage to the capital stock. And by capital stock, I mean the maximum breeding population consistent with the greatest number of young birds that can be brought to the opening of the hunting season.

Twenty-five years ago, the biological basis of hunting was thought to be a matter of simple arithmetic. "If satisfactory sport and a safeguarded breeding stock are desired on the same ground year after year," Stoddard (1931:226) wrote of bobwhite (*Colinus virginianus*), "the number of birds harvested by man must be offset by control of natural enemies, improvement of coverts, or restocking."

This thinking began to change in the 1930's with the intensive studies of predation by Errington (1935, 1937) in Wisconsin and Iowa. The theory emerging held that predators, parasites, disease, and/or food shortages act in a complementary fashion upon most wildlife populations and that one or more of these, acting in combination, level off the fall and winter surpluses produced each summer. Within this system, various kinds of predators likewise act in combination. In effect, then, well-regulated hunting should be able to remove a part of the fall population without affecting the population next spring (Errington 1935, 1936). The critical test of this hypothesis rests upon controlled experiments. Three of these have been carried out on bobwhites with generally confirmatory results (Errington and Hamerstrom 1935, Baumgartner 1944, and Mosby and Overton 1950).

Similar experiments by Glading and Saarni (1944) revealed that hunting does affect spring population levels of California quail (*Lophortyx californica*). The shot population, however, gained 97 and 109 per cent in two reproductive seasons in contrast to gains of 38 and 58 per cent on the control area. The principle governing this recovery phenomenon was Errington's (1945) rule that the percentage of summer gain tends to be inversely proportional to the density in spring. Harvests of 24 and 36 per cent did not affect density the following fall; a 38 per cent harvest did.

One should notice that the reliability of these tests of two biological principles underlying hunting harvests importantly rests on the amount of unrecorded ingress and egress of birds on the study areas. Errington and Hamerstrom (1935) felt sure that this occurred on some of the plots t[illegible] they censused, and for final and positive proof, Glading and Saar[illegible] [illegible]4) suggest the use of much larger acreages than the ones the[illegible] It is, I believe, understood that these two principles (hunting as a complementary component of predation; hunting-accelerated rates of reproductive gain) can and probably do operate together on game bird populations.

As to how these two ecological principles underlie our present system of harvesting waterfowl, I am afraid we usually do not know. This is basically due to the practical problems our field men face in censusing these highly mobile species and determining their productivity. The answers may be different for ducks and for geese, and in view of the relatively unstable water conditions on our prairies, they may not hold for any long period of years. The recent development of Flyway Councils clearly focuses the search for these answers on a broad regional basis, where it should be.

If moderate hunting pressure does not affect the population in spring, then the biotic factors limiting the population operate chiefly in late fall or winter. If the effect of hunting is not counterbalanced until the following summer or fall, then the biotic limiting factors operate on the breeding grounds. In the first case, a conservative hunting system takes a winter surplus; in the second it essentially *takes a spring surplus that is, in effect, reproductively inefficient.* The distinction between the two loses much of its importance when economic forces clearly or potentially set a limit to the number of waterfowl which a given breeding or wintering area can carry. It *takes on practical importance* when management can offset the operation of a limiting factor by habitat development or curtailing the effect of some biotic factor.

At the present time, it seems likely that verification of the biological principle (or principles) underlying our harvest of waterfowl will occasionally come from straight deductive thinking and our experience with ecological phenomena that are taking place. The spectacular upward surge of the Canada goose (*Branta canadensis*) population in the Mississippi Valley in recent years is, in the opinion of many waterfowl workers, almost certainly a response to the increased carrying capacity of this range, largely brought about by the wide-scale use of mechanical corn pickers in that region. This change in population level must mean that conservative harvesting of this species, when it did take place in the past, operated on the principle of complementary predation.

The picture for Mississippi Valley mallards (*Anas platyrhynchos*) is complicated by the increase of carrying capacity accompanying the expanded culture of domestic rice (Hawkins, Bellrose and Smith 1946) and by great losses of winter range brought on by the construction of levee systems by the Corps of Engineers (Anderson 1955). While these two forces may not cancel each other completely, the northward march of agricultural drainage and clearing strongly implies (to some of us at least) that the mallard population in this great region is in general limited by its breeding range (Gavin 1953; Mair 1953, 1954).

If one turns to the *Atlantic seaboard*, one can find Wright (1948) concluding that there is no need for habitat improvement or predator control on the waterfowl nesting grounds in eastern Canada; while, on the other hand, Williams (1950) believes that management must concentrate on *a small-marsh-development program* in the northeastern states and southeastern Canada. If Wright's conclusion is valid, conservative harvesting in this flyway is today based on hunting that is a complementary component of winter predation, winter *diseases and* the like. If Williams' belief is correct, conservative hunting in this region serves to increase rates of summer gain.

Still another school holds that local populations in the interior are being wiped out by hunting pressure (Hochbaum 1947). It is against this background that the need of management for more facts on waterfowl has been repeatedly stressed by writers and speakers on waterfowl: by Cottam (1949), Bellrose (1950), Mills (1951), and Olds and Swift (1953). As the latter emphasized two years ago, the states are willing to assist in waterfowl management with all their resources, but they need to know first whether the present breeding and wintering grounds in each flyway are being fully used by the birds.

Having thus found an academic question closely interwoven with a practical management problem, I think we can answer our original question about the biological principles underlying waterfowl harvests as follows: Scattered and rather preliminary research on upland game birds suggests the existence of two principles: (1) that conservative hunting is offset by decreases in natural mortality in winter and (2) that such hunting can also be offset by automatically increased rates of reproductive gain. These quite possibly have different degrees of application for ducks and geese. The differences may also extend to individual species as well as to regions or flyways. At the present time they are not clearly identifiable with the *harvest* of *many* waterfowl populations.

### WHAT FRACTION OF THE POPULATION CAN BE HARVESTED?

*As far as I know, this question was first raised at a* North American Wildlife Conference by Harlow Mills in 1951. The question is a challenging one, especially when we realize that modern research has repeatedly disclosed how many species of fish and game were being underharvested under modern conditions.

Efforts to turn the art of harvesting game-bird populations into more of a science have necessarily awaited development of a reliable system of forecasting population levels available for hunting. Such a science developed rapidly around the ring-necked pheasant (*Phasianus colchicus*), but harvest principles based on a sexually dimorphic polygamous species (D. L. Allen 1947) are for the most part different from those that can be set up for monogamous species like waterfowl.

At our present stage of research and development, we are moving into a position where we can determine just what fractions of certain populations are taken by hunters each year. The trend of inquiry is proceeding along two lines. On the one hand, there are new and increasingly precise measurements being made of the harvest. At least for some species in which the population can be accurately inventoried in January, these kill estimates can be converted into fractions of the population. Thus for Canada geese, Hanson and Smith (1950:192) were able to estimate that a hunting kill of 37 per cent and natural mortality of 18 per cent dropped the Horseshoe Lake population 28 per cent from 1944 to 1945. One year's production could make up for a 27 per cent total mortality; it could not make up for a 55 per cent drain.

The second line of inquiry utilizes raw banding data in which hunters furnish all or most of the reports of recoveries. Attempts to convert these recovery rates into percentages of the birds that were bagged in each flyway have been held up by (1) the failure of some hunters to report bands (Leopold 1933:156), (2) by the early emphasis on banding during the hunting season, and (3) by the probability that trapped samples are not randomly selected components of large regional populations (Hickey 1951, Crissey 1955). I would like to review here briefly the published progress that has been made in this connection on mallards in the Mississippi Valley.

Bellrose (1945, 1955, and Bellrose and Chase 1950) has been particularly aggressive in clarifying the first of these variables for mallards banded in Illinois. It is now apparent that, for every 10 bands reported for Illinois-trapped mallards, at least 21 banded birds are actually bagged; and, as Bellrose (1955) points out, the actual figure bagged must be higher. As a working approximation of the correction factor for this variable, I use 2.3 times the reported recovery rate. This may be regarded as a minimal estimate, and Bellrose (1955) has suggested that correction factors of even 2.5 or 3 are possible.

Over an 8-year period, 9.9 per cent of DU's summer-banded adult mallards were shot and reported by hunters in the hunting season that immediately followed (Hickey 1952:127). If we multiply this figure by 2.3 for unreported bands, we have 22.8 per cent of this sample bagged by hunters. This represents, of course, a minimal estimate and stands in contrast to $11 \times 2.9 = 32$ per cent calculated for Illinois mallards by Bellrose and Chase (1950).

Now this 22.8 per cent is simply an estimate of an 8-year average of mallards bagged. The magnitude of annual variations in some samples of the mallard population may run as high as 35 per cent for a North Dakota refuge in 1939 and as low as 13 per cent for some of DU's birds in 1941 and 1943 (Table 1).

This banding technique and other sampling methods for determining the hunting kill still require a further estimate of unretrieved cripples that ultimately die from gunfire. Our past reliance on hunters' reports of birds knocked down has permitted us to compare crippling losses from different types of shooting and from different places. While Bellrose's (1953b) tabulations of the kinds of wounds encountered in bagged, crippled and trapped birds possibly open the

TABLE 1. MINIMAL ESTIMATES OF PERCENTAGES OF BANDED MALLARDS BAGGED IN THE SAME SEASON IN WHICH THEY WERE BANDED[1]

| Where Banded | Canada | N. Dakota | NE. Ill. | Ill. River |
|---|---|---|---|---|
| 1939 | .... | 36 | .... | 7 |
| 1940 | 21 | 33 | .... | 16 |
| 1941 | 13 | 20 | .... | 6 |
| 1942 | 15 | .... | 18 | 13 |
| 1943 | 13[2] | .... | 12 | .... |
| 1944 | 25[2] | .... | 23 | .... |
| 1945 | 24[2] | .... | 20 | .... |
| 1946 | 22[2] | .... | 16 | .... |
| Banders | DU Canada | Hammond Henry | Jedlicka | Bellrose |

[1]These birds include both adults and juveniles; correction factor of 2.3 used for unreported bands; band recovery rates taken from Bellrose (1944) and Hickey (1952:127).
[2]Summer-banded only; all others include birds trapped during the hunting season.

door to a statistical evaluation of this phenomenon, it more and more looks to me as if we will eventually have to compare the bag at checking stations with the numbers of cripples that our dogs can pick up immediately after the opening weekend of a season.

For the present, I am willing to take hunters' reports of cripples only as a crude and wholly preliminary estimate of this added drain on the population. Bellrose (1953b) has tabulated reported cripples from six states and shown that these average 22.5 per cent of the birds found in hunters' bags at checking stations. According to my reckoning (22.8 per cent bagged times 22.5 per cent), this represents 5.1 per cent of the population being studied. This figure is so small that any further guesswork about which cripples recover is academic in our review. I assume the cripples to be lost. The total average drain on these banded segments of the population is about 28 per cent.

With these reservations in mind, I think it is instructive to tabulate what little we know statistically about current rates of harvesting monogamous game-bird populations in North America (Table 2). Because the extent of nonhunting mortality is virtually unknown in game birds, I have substracted known or recommended hunting-mortality rates from average adult over-all mortality rates to see what non-hunting rates are like. The subtraction is defensible only if one subtracts averages from averages. Lines 1 and 4 refer to hunting mortality on experimental areas and should be regarded as exploratory only. Line 7 compares an approximate adult mortality for a supposedly stable population of Canada geese with hunting and nonhunting rates calculated by Hanson and Smith (1950:192) for a specific year when the population decreased 28 per cent. A second limitation to this comparison is imposed when (a) the hunting-mortality rates involve both adult and juvenile components of the

TABLE 2. HUNTING AND NONHUNTING MORTALITY RATES (REAL OR ESTIMATED) IN MONOGAMOUS GAMEBIRDS

Annual over-all mortality rates (a) here represent averages. Hunting mortality rates (b + c) tend to refer to study areas. The nonhunting mortality rates (d), obtained by subtracting (b + c) from (a), can only be regarded as rough estimates of the magnitude of this phenomenon. This subtraction was not carried out for the Canada goose.

| | (a) | (b) | (c) | (b + c) = | (d) a — (b + c) | |
|---|---|---|---|---|---|---|
| Species | Average Over-all Adult Mortality Rate | *Annual Hunting Mortality* Per Cent Bagged | Per Cent Crippled | Sub-total | Annual Nonhunting Mortality | Ref. |
| 1. Bobwhite | 83 | | | 38 | 45 | 1 |
| 2. Bobwhite | 83 | | | 50 | 33 | 2 |
| 3. Blue-winged Teal | 57 | 7 | 2 | 9 | 48 | 3 |
| 4. Calif. Quail | 50 | 25 | 11 | 36 | 14+ | 4 |
| 5. Mallard | 48 | 32 | 9 | 41 | 7 | 5 |
| 6. Mallard | 48.7 | 23 | 5 | 28 | 20.7 | 6 |
| 7. Canada Goose | ca. 40 | 28 | 9 | 37 | 18 | 7 |

References

| | |
|---|---|
| 1a, 2a, 4a | *Estimated from age-ratio data by Hickey 1955* |
| 1 (b + c) | One-year experiment by Mosby and Overton 1950 |
| 2 (b + c) | Generally recommended figure (Davison 1949:134-135) |
| 3 | Bellrose and Chase 1950 |
| 4b, 4c | Glading and Saarni 1944 |
| 4d | Obtained by subtraction; since the experimentally shot population dropped below that of the control in March, nonhunting losses were probably higher than 14 per cent. |
| 5 | Bellrose and Chase 1950; *Illinois-banded birds* |
| 6a | Hickey 1952:159 |
| 6b, 6c | As explained in the text |
| 7a | Round figure taken from Fig. 80 in Hanson and Smith 1950:160. This refers to birds banded in 1926-1932. |
| 7 (b + c), d | *Calculated for 1944-1945 for the Horseshoe Lake flock by Hanson and* Smith 1950:102; population decreasing. |

population and (b) the juveniles are killed at a higher rate than are the adults. This may limit the significance of the nonhunting rates derived here for California quail.

My own calculation of an average hunting-mortality rate for mallards differs quite importantly from an early one estimated by Bellrose and Chase (1950) and Bellrose (1953b). The difference in the two estimates principally arises from a choice in the correction factor to be used for unreported bands. I am not sure how much Bellrose would revise his first estimates in the light of his recent findings (1955) on the failure of hunters to report bands they recover.

On a previous occasion (Hickey 1952:79), I used age ratios and banding data in an attempt to determine the percentage of redheads (*Aythya americana*) shot by hunters. The results for both adult and juvenile components of the population are limited by the time period of the banded sample used (birds banded 1926 to 1935), and by my failure to represent the age ratio in hunters' bags in an accurate and realistic manner; I believe these results should be rejected.

As a general rule, I think we can observe that hunting mortality rates must bear some relationship to over-all mortality rates if hunting itself functions as complementary predation. In species that are heavily cropped, the hunting mortility rate is at least one-half of the over-all annual mortality rate. This can be noticed in Figure 1 in which I have emphasized our best available estimates of average conditions.

On the whole, it would be better to add two more points to this graph before we base any hard and fast conclusions on it. The biological evidence currently available suggests to me that we could probably harvest 32 per cent of our adult blue-winged teal (*Anas discors*) instead of 9 per cent (estimated as the current harvest for Illinois-banded birds by Bellrose and Chase 1950). It also seems possible that 20 per cent would prove to be a conservative harvest figure for adult Canada geese. While higher harvesting rates presum-

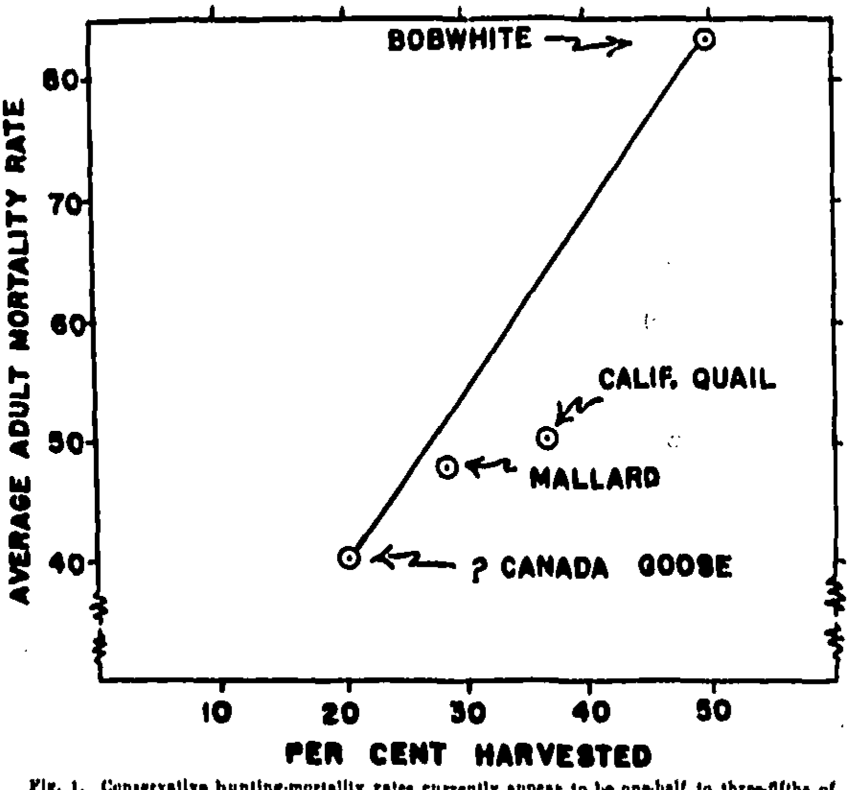


Fig. 1. Conservative hunting-mortality rates currently appear to be one-half to three-fifths of the average over-all adult mortality rate in monogamous gamebird populations. These hunting-mortality rates include crippling losses. A more instructive comparison would relate hunting-mortality to over-all mortality rates for both adult and juvenile segments of the population.

ably apply to juvenile birds in waterfowl populations, it is difficult at the present time to make any preliminary generalizations about them. This is particularly regrettable in the case of Canada geese (where juveniles are so vulnerable to the gun), but perhaps not critical for our purpose. From a table in Hanson and Smith's monograph (1950:192), I would judge that a 19 per cent harvest of the population would have kept it in balance in 1944-45.

I think the meager information assembled here suggests that DU-banded adult mallards, which formed the basis for my calculations of hunting mortality, were harvested in the early and mid-1940's in a conservative manner analagous to recommendations offered for bob-white quail by Davison (1949) and for California quail by Glading and Saarni (1944). Mallards banded on the Pacific Coast in the 1920's sustained average over-all mortality rates of 64 to 67 per cent per year (Hickey 1952:145a). Since this occurred at a time when bag limits averaged 26 birds and the season lasted three months, there is a strong implication that the increase in over-all mortality over modern rates of 45-49 per cent per year was due to hunting. In the absence of population indices in the 1920's, it is now difficult to prove that high harvest rates of the order of 45 per cent mean a declining mallard population. Judging from what we know of quail, hunting mortality of this magnitude is out of line for the mallard.

Hochbaum (1947) has described how 1946 was the first year in which he could find perfectly suitable breeding habitat in the Lake Manitoba basin, without breeding waterfowl. Since the kill there in the following autumn was the heaviest on record, he believed that overshooting was locally responsible for the decline in breeding ducks. In 1939 and 1940, first-season band-recovery rates of 15 and 14.2 per cent for mallards trapped by Hammond and Henry at the Lower Souris National Wildlife Refuge in North Dakota far exceeded the average of 9.9 per cent obtained for the birds DU banded in Canada (Hickey 1952:127). If the familiar unreported band bias and crippling losses associated with such recovery rates are of the same order of magnitude as those reported for Mississippi Valley mallards by Bellrose (1953b, 1955), I believe we would have to conclude that this North Dakota segment of the population was being overshot. These two pieces of evidence lead one to infer that our modern hunting regulations are on the average permitting us in the interior of the continent to harvest close to the maximum percentage of the mallard population biologically possible. The margin is, in fact, so close that the maximum is occasionally and locally exceeded.

Band-recovery data (Table 3) demonstrate that we are harvesting

TABLE 3. SOME BAND-RECOVERY RATES FOR NORTH AMERICAN WATERFOWL

| Species | Banded in North America[1] No. Banded 1923-46 | Per Cent Recovered to 1946 | Banded by D.U. 1939-50 Per Cent Recovered to 1950[2] | Banded in Illinois Per Cent Recovered First season Only[2] |
|---|---|---|---|---|
| Gadwall (*Anas strepera*) | 5,415 | 17.1 | 13.4 | 11 |
| Canada Goose (*Branta canadensis*) | 15,076 | 13.7 | .... | .... |
| Mallard (*Anas platyrhynchos*) | 250,986 | 15.0 | 15.0 | 6.4 |
| Black Duck (*Anas rubripes*) | 96,334 | 12.8 | .... | .... |
| Redhead (*Aythya americana*) | 12,987 | 12.8 | 18.0 | 8.0 |
| Baldpate (*Mareca americana*) | 10,163 | 12.7 | 12.6 | 6 |
| Shoveller (*Spatula clypeata*) | 4,541 | 10.4 | 9 | 8 |
| Greater Scaup (*Aythya marila*) | 2,851 | 10.3 | .... | .... |
| Pintail (*Anas acuta*) | 172,000 | 10.2 | 9.1 | 6.0 |
| Ring-necked Duck (*Aythya collaris*) | 22,061 | (10.3) | .... | 5 |
| Blue Goose (*Chen caerulescens*) | 1,000 | 7.0 | .... | .... |
| Canvasback (*Aythya valisineria*) | 5,390 | 7.6 | 14 | .... |
| Green-winged Teal (*Anas carolinense*) | 41,329 | 6.7 | 5.5 | 5 |
| Lesser Scaup (*Aythya affinis*) | 40,770 | (5.7) | 9.4 | 5 |
| Wood Duck (*Aix sponsa*) | 9,277 | 5.5 | .... | 5.4 |
| Cinnamon Teal (*Anas cyanoptera*) | 2,074 | 4.9 | .... | .... |
| Blue-winged Teal (*Anas discors*) | 40,357 | 4.6 | 3.9 | 2.4 |
| Ruddy Duck (*Oxyura jamaicensis*) | 900 | 3 | .... | .... |
| Source | U.S. Fish and Wildlife Service banding files, 1947 | | Cartwright and Law 1952 | Bellrose |

[1]An explanation of the derivation of these data as well as some reservations about their interpretation is given in Appendix 1.

[2]Percentages not carried past the decimal point refer to samples of less than 1,000 birds banded.

other species of ducks at markedly lower rates. For the most part, there is no doubt that many of these other species could biologically sustain markedly higher rates of hunting mortality than the ones these recovery-rates imply. Whether or not such a change could be effected in practice remains to be seen. This leads us to our next question.

## HOW FAR CAN WE REFINE OUR HARVEST SYSTEM?

If any further proof is needed that regulation of our waterfowl harvest is an art rather than a science, one need only point to the scarcity of published research work evaluating bag limits and other prohibitions for their effect upon the mortality (or survival) of waterfowl. As Mair (1954) points out, our knowledge of the effect of specific regulations is still generally qualitative and not quantitative.

Modern duck-hunting differs so much from goose hunting that we can consider the two as separate harvest systems in many parts of the continent. The duck kill is significantly characterized by the manner in which it is dispersed over great areas; the goose kill often is locally concentrated. Let us first consider the effect of regulations on ducks.

***Ducks.*** The most thorough and objective of the published evaluations of regulations have been those carried out in Illinois by Bellrose (1944) and in Utah by Van den Akker and Wilson (1951). There are three significant conclusions brought out in each of these studies: (1) bag limits had little effect upon the kill when set above a certain point; (2) although short seasons are probably more effective in limiting the kill, the total bag does not seem to be proportional to length of season; (3) setting special bag limits for different species as we did in the 1930's and 1940's is a technique of doubtful effectiveness.

In flyways where biological rather than economic limits govern the rate at which we want to harvest our ducks, harvest regulations will apparently have to reflect the general failure of duck hunters to identify many of their birds before they are shot. This is, I think, a generally accepted condition which is backed up by the Bellrose's (1944) research on the wood duck, the experience Van den Akker and Wilson (1951) report on protected diving ducks, and Elder's (1955) conclusion that size of the target offered by each species in general determines the average rate at which each species of duck is now being harvested. There are, of course, specific behavioral or ecological factors that tend to abrogate the effect of Elder's rule: the pelagic feeding behavior of eiders (*Somateria* sp.) and old-squaws (*Clangula hyemalis*), the early movement south of the blue-winged teal, and the coincidence of the wood duck's breeding range with areas of intense hunting pressure.

With the mallard apparently bearing about the maximum rate of harvesting in the Mississippi Valley that it can sustain, and with local overshooting evident, a corrolary to Elder's rule would embrace the idea that any generalized attempt to increase the take on currently underharvested species in this flyway will jeopardize the main species at which the regulations are aimed.

I think the rate of harvest could be safely increased on an underexploited species like the blue-winged teal by means of special hunting seasons which would have to be confined to areas (a) south of the breeding range of the mallard and perhaps of the black duck, and (b) away from waters frequented by the wood duck. This is a pretty large order. Perhaps the first step toward it would be an intensive management program in the South to raise the wood duck population to a point where it could sustain hunting pressure from regulations definitely designed to harvest more blue-winged teal.

The selective raising of harvest rates is a tough problem not because of its biological aspects but because of its human relationships. What waterfowl managers need today is a public acceptance of the professionals' freedom to experiment in this connection and a public willingness to allow such experiments to be carried out locally without reference to "penalty days" and the like.

I think it is well to ask at this point if a harvest system can ever be precisely designed for our principal game ducks like the black duck, mallard, and pintail. Can it ever be really "scientific"?

Nearly every duck hunter is aware of the effect of weather on his bag, and this is particularly true of hunting conditions where migrants are being shot. In the Lake States, conservation-department estimates of the state-wide kill show varying degrees of synchronization, as Figure 2 demonstrates. The year 1947 was a generally poor one for

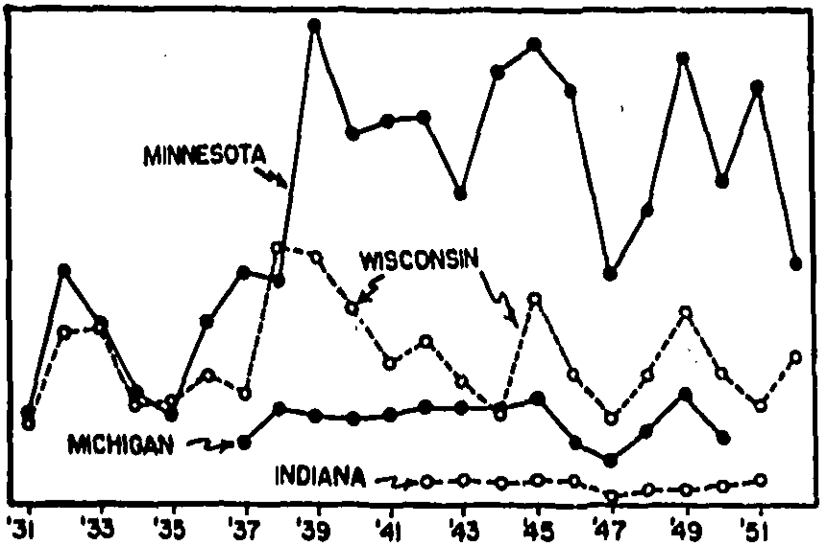


Figure 2. Estimated waterfowl kill in four states in the Middle West

wildfowlers in this region, but in the absence of good population indices, the effect of weather on the kill is not evident.

Weather effects are, I think, evident in the variations that one finds in first-season band-recovery rates (the number of birds reported shot in the same season in which they were banded divided by the number banded). This is clear in Figure 3 in which you will notice that the kill in 1941 and 1943 failed to bear any relationship to length of season as established in the previous year.

What waterfowl hunting regulations do is to hold for average conditions. Restriction of the length of season has, on the average, been associated with a 50 per cent reduction in band recovery rates (Bellrose 1944). It has also been shown to have coincided with a marked

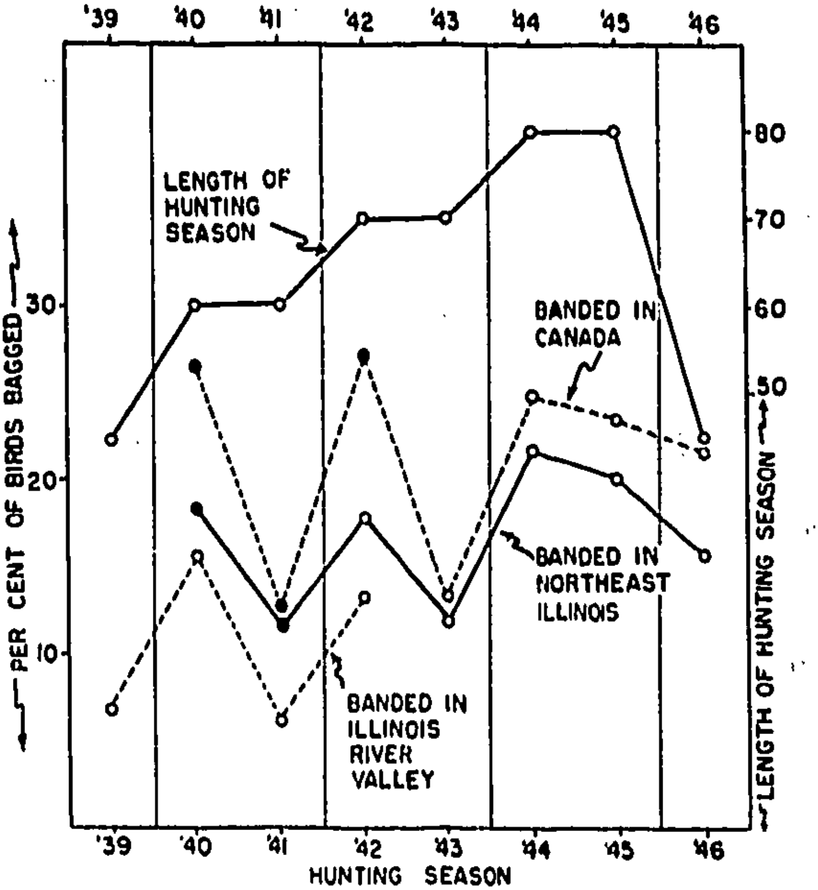


Fig. 3. Relation of length of hunting season to the percentage of unaged mallards bagged in the same season in which they were banded.

reduction in average over-all mortality rates of adult mallards on the Pacific Coast, from about 64 per cent per year to less than 50 per cent (Hickey 1952:144). The point is not that regulations are ineffective but rather that they hold for average conditions. They are an important adjunct to the art of managing the waterfowl crop. Under the present system, a regulations committee makes every attempt to utilize the best information available about the size of the duck population that is expected to start south. If you will recall the present vagueness of our 30-day weather forecasts, you will appreciate our current inability to fix or forecast the duck kill with real precision in many parts of the country 60 to 120 days later.

Does this mean that we should abandon a lot of our summer inventories and let an assumed average population level be the basis of averaged-out hunting regulations? One of the most remarkable waterfowl discoveries of the past decade centers in the very great fluctuations in the productivity of mallards encountered by Bellrose and Hawkins in Illinois and Arkansas (Hawkins, Bellrose and Smith 1946). In delaying the annual announcement of waterfowl regulations each summer, the Fish and Wildlife Service attempts to keep abreast of these fluctuations. In baseball terminology, it is playing the percentages. For the Service to abandon the inventory work so ably developed thus far would, it seems to me, be akin to a baseball manager failing to keep track of the number of balls and strikes.

*Geese.* Many goose populations in North America today exhibit three of the four requisite characteristics which make a scientific management of their harvest possible. They have relatively narrow migration routes and well-delineated breeding grounds that can often be associated with precisely defined wintering areas. The breeding population lends itself to an actual count (in this case on the wintering grounds). The kill, being largely concentrated at a few places, lends itself to weekly enumeration. What is absent is a good index of breeding success, or at least an index that can be taken into consideration by a regulations committee.

At the present time, goose populations are largely harvested according to the number we count in January and an educated guess as to their probable productivity. I think that we are definitely moving toward goose seasons in which the kill will be fixed as so many thousand birds, that the U.S. Fish and Wildlife Service may have to set the kill for each state or rather for the sum total of the principal harvesting places in each state (perhaps as a percentage of the duck stamps sold in that state during the preceding year), and that seasons will remain open until the set kill figure is reached and no longer.

The biological obstacle to such a system is, I think, surmountable, despite the prohibitive costs of running aerial transects for broods across the tundra. We can, I think, make our regulations in August, sample productivity at September stopping-off places, and modify the regulations well before geese hit the big refuges in the states in October or before the kill mounts in November. Such a system, to me, implies controlled goose shooting at all the major sites of the goose kill today. The margin of error in the final kill outside such places will not be large. Managed goose hunting as it is appearing on an experimental basis at places like Horicon, Wisconsin (Jahn, Gunther, and Bell 1954), certainly holds the promise that this segment of our waterfowl population will eventually be harvested on a scientific basis.

In attempting to answer here the qustion set for me by the program committee of this conference, I have of necessity avoided any discussion of the esthetics of hunting waterfowl. To many these are fully as important as the size of their bag, and they look with a jaundiced eye on controlled-hunting programs. It is true that such programs importantly break with traditional patterns of hunting that we like to think of as our heritage, but it is manifestly unrealistic to look for unrestricted wildfowling in the face of a rapidly mounting human population.

## ACKNOWLEDGMENTS

The current exchange of ideas between waterfowl workers is so rapid and informal as to make acknowledgments difficult in a paper of this kind. I am certainly indebted to A. S. Hawkins of the U.S. Fish and Wildlife Service, for unpublished data and fresh ideas; and to F. C. Bellrose of the Illinois Natural History Survey, whose published papers have made a review like this one possible. C. S. Robbins at the Patuxent Wildlife Research Refuge was extremely helpful in tabulating band recoveries for me. To S. T. Dillon, Alex Dzubin and R. A. McCabe of the University of Wisconsin, I am also indebted for specific advice and criticism.

## SUMMARY AND CONCLUSIONS

Waterfowl management in North America is in part an art, based on common sense and experience, and in part a science, based on a systematically derived set of principles. Management of waterfowl by means of refuges, by the curtailment of botulism, by the elimination of poachers and predators, by the creation of new habitat or the development of better habitat is essentially an art to which technology has been making increasing contributions. These components of the program vary in the amount of precise knowledge available to the waterfowl manager. Each originated as an art and is progressing toward refinement as a science.

Attempts to regulate the waterfowl harvest involve somewhat the same evolutionary development. There are sharply different problems as to how we can harvest ducks or geese; and in the degree to which each can be placed on a scientific basis. The biological basis for harvests by hunters appears to rest on the principle that conservative hunting reduces winter mortality from natural causes or results in increased rates of reproductive gain in the following summer. These principles are not too well documented in upland game bird research, and they will be hard to verify experimentally with waterfowl. At the present time, it appears that conservative hunting can take a fraction equal to about one-half of the annual over-all mortality rate.

Rates of hunting mortality are relatively high in the mallard and black duck, intermediate in the diving ducks and lowest in the teal. About 28 per cent of the adult mallard population in the Mississippi Valley area is estimated to die from hunting in an average year. This appears to be about as much as the population can stand, but under present conditions the hunting-mortality rate will importantly fluctuate from year to year and locally be excessive. The governing variable for a given year is weather which can so influence the migration of some ducks as to double the rate of harvest of one year over another or reduce it 50 per cent even with the same set of regulations. This is true of mallards, probably of pintails, and may or may not apply to relatively consistent migrants like bluebills and coots (*Fulica americana*). Modern bag limits and seasons are in this sense set up on the basis of average conditions. They are relatively crude management tools which are known to be effective in the long run. The dispersed character of duck hunting seems to preclude any immediate possibility of attaining a weekly autumnal index of the success of our hunters and our regulations; for our more erratic migrants, annual changes in season-length and bag limits remind one of a set of conference rules requiring football coaches to name their starting line-ups in August and to stick to these without substitution from October through December.

Possibilities for managing the harvest of geese on a scientific basis are more encouraging. In Mississippi Valley geese, we already know the relation between regional nesting grounds, migration routes, and winter areas. We lack a summer index of annual productivity, and we may have to settle for some moderately inexpensive one determined at September concentration points. To place the goose harvest on a scientific basis, administrators will thus need to enjoy greater flexibility in setting regulations; they will also need authority to designate the total goose kill in each state and to modify their regulations, if necessary, during the course of the hunting season. At the present time, the goose harvest is being regulated on the basis of winter inventories, experience with recent regulations, and with due regard to potential damage to cropland by the geese on their winter range. The conference rules permit the coaches to name their line-ups in

August on the basis of spring practice and the behavior of last year's performers. This is working reasonably well in goose management today, as the recent recovery of Canada geese in the Mississippi Valley testifies. Our present program does not, however, endeavor to harvest a precise percentage of the population.

One's over-all impression in this review is that the federal bureaus are doing a pretty good job in setting regulations. The real promise of scientific management lies, it would seem, not so much in refinement of the harvest as it does in the improvement of waterfowl habitat.

## APPENDIX 1

The numbers of banded ducks and geese have been routinely catalogued each year by Fish and Wildlife Service personnel since 1923. Table 3 contains the number of birds reported banded from 1923 to June 30, 1946, inclusive. A small error occurs here due to birds which were banded in say 1945-46 and not reported as banded until later. Because they probably were not typical of the behavior of wild-trapped waterfowl, I eliminated from this series 21,702 hand-reared birds and a small number of individuals involved in homing experiments.

The totals for the numbers recovered were obtained by discarding noticeable runs of reports of trapping accidents or shooting reports sent in by a bander. This standard served to eliminate a rather large series of birds banded by E. A. McIlhenny at Avery Island, Louisiana, and reported by him as shot in the vicinity. (McIlhenny sent in 45 per cent of all the reports of banded ring-necked ducks shot in North America.) In addition to these 4,225 records, I also separated out 24,139 reports of birds retrapped by the banders. The number of waterfowl recoveries left in the banding files of the Fish and Wildlife Service from 1923 to June 30, 1946 was then obtained and approximate recovery rates were computed as of the end of this period (Table 3). The result approximates the relative rates at which these species were harvested. A better index would of course involve only the percentage of birds shot in the first hunting season after they were summer-banded.

When the recoveries were further extended to include the five fiscal years ending on June 30, 1951, the mean recovery rate on 16,643 geese (4 species) was raised from 13.1 to 18.0 per cent. This change presumably reflected the active Horseshoe Lake, Illinois, banding of H. C. Hanson near the close of the 1923-46 period. For 17 species of ducks involving 212,507 birds the mean recovery rate similarly changed from 7.5 to 7.8 per cent. I am indebted to Chandler S. Robbins of the U.S. Fish and Wildlife Service for ascertaining these changes.

The rates reported here are subject to certain real or potential errors that force one to regard the percentages as a crude index only. A time barrier prevented me from rechecking the annual Service tabulations for clerical errors in the numbers of birds banded. The summation of recoveries included a number of duplicate recovery reports for all species except the mallard; in a sample of 801 waterfowl cards that I handled in this period, 3.1 per cent were duplicates. These as well as non-hunting records included in the tabulation can be regarded as minor and fairly constant sources of bias between each species. (Many reports of birds "found dead" or "injured" must of course reflect hunting phenomena.) Variability between species is due not only to sampling size, as these figures imply, but also to the season of banding. Recovery rates are presumably lowered when adult birds are banded on their wintering grounds, and increased when juveniles are marked just before they start their first flight south.

The North American banding system is defective in representing the hunting reports of Indians in Canada and persons south of the Mexican-United States boundary. Any index based on banding reports thus holds principally for literate white hunters; fluoroscopic tests of the frequency of birds carrying body shot presumably are necessary to disclose the degree of shooting pressure directed upon waterfowl by non-English-speaking peoples.

It should be obvious that these recovery rates may vary not only regionally but from year to year as well. In general, most of the birds cited here were banded in the United States during the late summer or in the autumn. It should also be obvious that the numbers of banded waterfowl reported here are not necessarily randomized samples of the continental population. The banding work on mallards, pintails and black ducks has been fairly well distributed over those parts of the United States in which these species normally range. For a few species, however, the banding operations have been geographically restricted. Thus, the more important banders of green-winged teal have been in the west (A. J. Butler in British Columbia, Malheur National Refuge in Oregon, and the Bear River Migratory Bird Refuge in Utah). Over one-quarter of the blue-winged teal recoveries reported here come from McIlhenny's banding work in Louisiana. The ring-necked duck and lesser scaup data likewise reflect McIlhenny's trapping success with these species as well as my deliberate elimination of so many of his recovery reports.

I have no explanation to offer for the surprisingly high recovery rate reported here for gadwall. The number of recovery cards for this species was checked twice. The result should, however, be regarded with some reserve. The high recovery rates for some geese could be biased by the greater cooperativeness of hunters in reporting trophy game. This point perhaps can be settled by fluoroscopic comparisons of the frequency of geese and ducks carrying body shot.

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## DISCUSSION

VICE-CHAIRMAN ANDERSON: Although this paper is rather scientific in its nature, it has a very practical application. For example, at the Mississippi Flyway Council meeting last Sunday the statement was made by one of the state representatives that we have no evidence at all that we could not safely harvest far more mallards than we do now. I hardly see how that gentleman could make that statement in the light of the data which have been presented by Dr. Hickey.

Are there any other comments or questions of Dr. Hickey on this subject?

DR. H. F. LEWIS [Nova Scotia]: I think it is very encouraging to hear Dr. Hickey say that he sees no hope for the scientific harvest of ducks. [Laughter] On the other hand I'm afraid I can't agree with him that the art of wildlife management is becoming a science. That appears to be an idea that they hold in Madison. However, I myself differ with it. [Laughter]

All the dictionaries that I have been able to get hold of say that when you apply a science that application is an art. Therefore, if the art were to become a science, it would no longer be applied, and would not be useful.

There is an abundant application for wildlife science, but its application, unless we are to rewrite the English language, must remain an art.

I must say that I think it's encouraging that he sees no hope for the scientific harvest of ducks, because after all I don't think we should forget, nor should we allow these keen, lean greyhounds of science to lead us to forget [laughter], that we are all serving a sport. The harvest of waterfowl and other game birds should be a sport, and it should depend to a very large degree on chance and individual skill.

DR. COTTAM: I should like to ask my friend Joe to what extent these principles that he has applied so splendidly in this paper to waterfowl apply also to upland game?

DR. HICKEY: Dr. Cottam, the principles that I meant are essentially brain children of Paul Errington. They were derived from a series of papers beginning in 1934 and 1935. In giving a presentation of this type I did not mention bibliographical titles, but the principle that hunting is a compensatory form of predation appeared in 1935 for bobwhite quail.

The other principle is found in Errington's paper of 1945. This is sometimes called the inversity principle—that the rate of reproduction gain in a population is inversely proportional to its density in the spring. It just happens that we have just a very few sets of experiments which show that these things actually apply to upland game birds. It's amazing that we don't have more.

VICE-CHAIRMAN ANDERSON: Did I understand you correctly that 28 per cent of the adult mallard population is removed annually by hunting, and that that is in your opinion about all that the population can stand?

DR. HICKEY: I'd like to answer that question this way, that based on what has been found out from some rather preliminary studies of upland game birds, a harvest of 28 per cent of the adult population of mallards is conservative. This does not take into consideration juvenile birds. We are certainly harvesting higher fractions of the whole mallard population than at the rate of 28 per cent, and I'm pretty sure that that is what has taken place in the samples that I studied.

Now, I want to emphasize also that this took place in the early and mid-1940's. What others are finding out now it is up to them to report. That is essentially an historical part in my paper.

DR. COTTAM: I'd like to ask one other question. It seems to me that our aim should be to take all the available harvest that the supply will admit, and I think we all will agree on that fundamental principle.

In the Pacific Flyway if conditions develop to the extent that we don't have carrying capacity for the winter; if, for example, Tule-Klamath Lakes is eliminated—which is the single most important waterfowl habitat on this continent in my opinion—we can harvest up to 75 per cent of the ducks and not hurt them a bit, because nature will eliminate them anyway.

While I'm mentioning that, may I say this, that such rumors are reaching me—and I'm hearing them from many sources. For obvious reasons they are not brought out in the open—they can't be. If the rumors are even half correct, some high powers are selling down the river Tule-Klamath Lake, which is the single most important area on this continent for ducks. If that area is drained and opened for homesteading, I think you can kiss good-bye to 75 per cent of the ducks in the Pacific Flyway, and it won't matter if you shoot them, because nature will eliminate them anyway.